

What's the Use of International Relations?

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What's the use of International Relations?

MICHAEL NICHOLSON¹

Abstract. This article discusses some controversies over the relations of the academic discipline of International Relations to policy. The article argues that, while our ultimate goal may be to have some impact on the world, the immediate goals can be more abstract. International Relations consists, and should consist, of a set of Lakatosian Research Programmes, some conflicting, some complementary, which range from the abstract to the directly policy-relevant. Effective intervention in social systems depends on analyses in more than common-sense terms. The article looks sceptically on recent disciplinary history, doubting both the supposed positivist hegemony and the supposed novelty of normative debate.

Introduction: varieties of influence

The purpose of doing International Relations, like all social science, is to influence people, sometime, somewhere in a context which will make a difference to their actions. Thus, at some stage, possibly distant, a course of action will be taken, or abandoned, as result of our efforts. The world will then look slightly or even significantly different because of our activities. We hope it will look better, though what 'better' consists of is itself a result of our moral positions. This also means that we think we have something to say which goes beyond that which a concerned citizen could say. I think, as a profession, we have such things we can say, though I shall not give a list of our rather modest achievements here.

A few years ago in York, William Wallace delivered the BISA lecture in which he urged us to go out into the big bad world and become engaged. He followed it up with an article in the *Review*.² He wanted us to become influential and speak truth to power. Ken Booth and Steve Smith responded.³ However, the whole concept of influence is more complex than any of them allow, and they fail to face some crucial issues sufficiently directly. I shall endeavour to do so here.

I do not want to imply that the improvement of the operation of the international

¹ This is an edited version of the lecture given at the BISA Conference in 1998. I have retained the informal, spoken style of the original. I wish to thank the following people who have commented on the original text of the lecture, the subsequent paper, or both; K.M.Fierke, Stephanie Hoopes, John Maclean, Caroline Nicholson, William Wallace and an anonymous referee. This is the first significant piece of work I have ever published which has not been read and criticized by Christine Nicholson who died in September 1998. I hope that her life-long insistence that I try to think clearly and take writing the English language seriously has had its effect.

² Published in a revised form as William Wallace, 'Truth and Power, Monks and Technocrats: Theory and Practice in International Relations', *Review of International Studies* 22:3 (July 1996), pp. 301–21.

³ Ken Booth, 'Discussion: A Reply to Wallace', *Review of International Studies*, 23:3 (July 1997), pp. 301–21; Steve Smith, 'Power and Truth: A Reply to William Wallace', *Review of International Studies*, 22:3 (October 1997), pp. 301–21.

system is the only reason we do International Relations. We instruct the young; though why we should bother unless International Relations were of some use is unclear. The discipline trains the mind, but so do others. If training the mind is central, I personally would recommend a joint degree in history and mathematics. Few seem to be interested in this suggestion. For our troubles, we get a secure if not extravagant salary and work in moderately congenial surroundings. While those set in authority over us are working hard to make us conform to current managerial dogma that if work is enjoyable, something must be wrong, there are still moments of pleasure in academic life.

But I come back to my main theme. International Relations is finally to be justified by the usefulness of what we have to say. However, while some of these things may be of immediate relevance, much will only be of relevance in the future and in just what form, if any, is hard to say at the moment.

I shall start with three points. I initially thought that they were too platitudinous to be expressed publicly until I realised that, even when mentioned, their implications had been largely ignored in the recent debate. First, influence is not just a question of advising policymakers. It ranges from this sort of direct involvement in policy (or indeed being a policymaker) to setting an agenda for a debate which may not become active for many years and certainly not of current relevance to the policy community. Secondly, when we are offering policy advice, who do we advise? Finally, International Relations consists of a broad range of topics with uncertain boundaries. Thus, it is practised by a group of people who need not all do the same thing and, indeed, should not be doing so. This last really is quite an extreme platitude which makes it all the more surprising that its implications are so widely ignored. These implications are less platitudinous.

According to Keith Webb's research,⁴ most BISA members want to advise the government. Collectively we feel a little hurt and neglected that we are not invited to quiet meetings in Whitehall to give our views on a variety of topics on which we think we are expert. No doubt some of us do get such invitations, though I do not. This is the most direct sense of influence.

However, there may be excellent reasons for us not to want to offer advice even if we thought anyone would be in the least bit interested in hearing from us. We may be working on things which will only come to fruition in the distant future. The work may be simply incomplete as the over-eagerness to apply game theory and related approaches to Vietnam suggests. We may be putting things on the agenda. Thirty years ago neither gender nor the environment were much spoken of though now both are central in the International Relations programme. Initially the scholars involved might have wanted to be part of a policy debate but, at the time, it seemed a remote possibility. At some stages, raising the issues is as crucial as providing the answers. As Keynes famously remarked, 'Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist'.⁵ From this it would seem to follow that one way of having

⁴ Keith Webb, 'Academics and Professionals in International Relations: A British Perspective', in Michel Girard, Wolf-Deiter Eberwein and Keith Webb (eds.), *The Theory and Practice of Foreign Policy Decision Making: National Perspectives on Academics and Professional in International Relations* (London: Pinter, 1994), pp. 82–94.

⁵ John Maynard Keynes, *The General Theory of Employment, Interest and Money* (London: Macmillan, 1936).

influence is to aim to become, in due course, a defunct economist. I shall return to Keynes from time to time in this article as one of the most successful cases of the academic policy adviser.

In the case of direct policy advice, we have to decide whom to advise which is determined by a mixture of deciding on the most effective way of achieving our political goals with which side in any dispute we favour. It may be the government of the scholar concerned. However, we may not like the government very much. We may then want to advise governments other than our own either because we think the sum of human welfare will be better if they get their way or because they are in a better position to achieve the ends we hold dear. We may wish to advise non-governmental bodies such as Amnesty or Greenpeace; more lucratively we may wish to advise Shell or BP. Some members of BISA have been involved in conflict resolution which is not at state level and only an extreme pedant would want to say that they were not doing 'proper' International Relations. Who we advise is an obvious question. It was actively and often acrimoniously debated in the earlier days of Peace Research. In a paper published in 1968, in which he attacked Johan Galtung, Herman Schmid⁶ started a vigorous and sometimes bitter controversy, though one which had been latent earlier. People were alert to such questions as when there is conflict, is conflict resolution always the best thing to do? Perhaps we should be committed and advise one particular side how to win. Further, moral actors may not always side with their own national government. These arguments took place in the days when, we are told, the discipline was so crushed by the positivist hegemony that there were no moral arguments going on.

The research community

To be engaged in policy is not merely worthy but necessary. It is a proper function for an academic to be an adviser. But it does not mean that we should all do it. The scholarly community is and ought to be a heterogeneous collection of people with very different ideas and very different interests. Another platitude you might think. However, we agree with this platitude more in theory than practice.

When we celebrate difference, we do so in many different ways and often through gritted teeth. The field of International Relations is a mass of Lakatosian Research Programmes.⁷ Some are just about different things. We can work in our own corner on, say, rational choice theory, without wishing to suggest that those who work on the problems of the Indian subcontinent are in any way mistaken or peripheral to the broader professional concern.

Other programmes compete more directly and involve different, incompatible approaches to analysing broadly similar questions. Thus, pluralists growl at neorealists in what I insist is a competition of commensurable though partially inconsistent research programmes, and not, as Michael Banks,⁸ followed by Martin Hollis and

⁶ Herman Schmid, 'Peace Research and Politics', *Journal of Peace Research*, 5 (1968), pp. 217–32.

⁷ Imre Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970).

⁸ Michael H. Banks, 'The Interparadigm Debate in International Relations', in Margot Light and A. J. R. Groom (eds.), *International Relations: of Current Theory* (London: Pinter, 1985).

Steve Smith,⁹ would have us believe, an opposition between a set of incommensurable and therefore necessarily noncompetitive paradigms. Outsiders agree that this is an excellent form of competition. Only those who are involved in the competing research programmes are willing to cry out about the folly of the grant-giving authorities, poorly advised, no doubt, by jealous opponents, when they throw their money away on a rival programme.

There are other programmes that we think are a waste of time, their practitioners working on a set of either trivial or artificial problems. Through forced smiles we publicly agree that a thousand flowers must bloom. Privately we wish that they would stop talking nonsense.

The point is, however, that we are a research community working over a wide range of topics, some close to policy interests, but many far away. The research community itself is just one of several interconnected research communities investigating the problems of human and social behaviour. William Wallace, along with many others, fall into what I would call the 'Popperian trap' of assuming that the research worker is all things at once. Popper seemed to think that all research workers combined the whole research community within each individual who proposed hypotheses, tested them and for whom true happiness came only when a treasured theory was shown to be false.¹⁰ The logic of discovery may have been illuminated but with the aid of some extremely bizarre views on both the psychology and sociology of research. The much more plausible account of Lakatos makes it all more credible by talking of a community and recognising the centrality of competition as the tool for the discovery of truth.

In an ideal world we would know everything, but the range of knowledge and skills required to investigate the social world means that no single scholar can work in more than a small handful of areas. Thus, to talk of the discipline as if it were some vague holistic entity being too close to policy or too far away is very misleading. Some people are close, and some are distant from policymaking. However, the various scholars in the various places are involved in a mass of partly competing and partly complementary programmes which, if they were all present in one person, or, indeed, in a few, would represent extreme psychological disturbance. It may well be true that some of us could broaden our horizons and work both at the policy end and the theoretical end of the spectrum. I would accept it as a criticism of the work I have done that it would have been enriched if I had been more overtly politically engaged. There is a limit, however, as to how far this can go.

There has, moreover, to be some fundamental research into the nature of the international system and, indeed, to the whole nature of human behaviour in order to back up those areas closer to policy. We should aim for evidence-based International Relations, not prejudice-based as a great deal is. To make claims for a special expertise where one does not exist can be both fraudulent and sometimes disastrous. Swaggering macho strategists should tend to their statistics before giving academic respectability to testosterone inspired acts of policy. The statistician behind them is a much humbler though necessary creature than these would-be

⁹ Martin Hollis and Steve Smith, *Explanation and Understanding in International Relations* (Oxford: Clarendon Press, 1991).

¹⁰ Karl R. Popper, *The Logic of Scientific Discovery* (London: Routledge and Kegan Paul, 1959); *Conjectures and Refutations: the Growth of Scientific Knowledge* (London: Routledge and Kegan Paul, 1968).

policymakers. Indeed, a statistician has been defined, no doubt by some innumerate opponent, as someone who wanted to be a chartered accountant but lacked the charisma. Such work, divorced from direct policy intervention but crucial for it to be effective, is a requirement. Someone has to do it.

From this we can see there is nothing inherently reprehensible in some parts of our community being involved in the specialist vocabularies which are sometimes necessary, or at least useful, to discuss some issue. Small subcommunities do not have to make everything comprehensible to everyone all the time. To do so would be very inefficient. There are technical arguments within subdisciplines. In retrospect much research is redundant, but it is hard and probably impossible to tell in advance which bits are going to be redundant and which not. Nor can we tell in advance which things are necessary steps in the arguments from which practical and applicable approaches develop from earlier, apparently abstract work. The abstract, abstruse, sometimes apparently pretentious, sometimes actually pretentious arguments which litter the professional journals are, for the most part, work in the process of development, not the final product. Most of it is irrelevant to the policy process. Its writers rarely pause to think what a Minister or Civil Servant would think of it. Nor should they. Out of such processes a little nugget of relevant work comes out which would not have been available if the complex process had not been gone through. Anyone who wishes can look in at the process as it is going on. It is not a proper question to ask of every stage and every step 'What's the use of this?'.

What is important is that, in due course, someone can express the findings of these subcommunities in language which is comprehensible to scholars working in other specialities, to the practitioner and to the concerned public. We do not need to hear of all the detours and false trails which some specialist area has taken. If people want to carry out their quarrels in the decent obscurity of a technical language then that is up to them. The more general scholar can wait for the expositors to synthesize and clarify the arguments as clarifications become possible and debates have moved to the stage where they are worth more general reporting. I am hostile to the view that technical arguments are always self-indulgent or, indeed deliberately exclusionary. Probably they sometimes are, but often they are the only realistic way of making progress in some specialist area. I would like to think that most things in our discipline can be stated at some stage in reasonably clear natural language, but this is not the same as saying that it is always practicable to develop these ideas without some specialist language and techniques.

This implies a particular view of International Relations, and of social science in general. There are things to find out about the social world which are not immediately obvious and, indeed, are sometimes obscure until illuminated. If not, and if International Relations as a discipline is just bluff commonsense, then it is not really a discipline at all. Policy advice becomes little more than the articulate expression of prejudice by clever people, and International Relations ends by being no more than a careful reading of the newspapers. They can read them in the Foreign and Commonwealth Office just as well as I can.

The term 'research community' implies a greater deal of mutual goodwill than is often justifiable. Not all is sweetness and light and the picture of busy scholars whose only goal is truth is a parody. I was shocked as a naive graduate student in economics in Cambridge in the 'fifties to realize that one half of the department did not speak to the other half. Like good debutante economists, the graduate students

formed many exotic theories as to why this should be, laced with the occasional unreliable fact. We debated eagerly whether the faculty's animosities were based on their technical differences, ideological differences or, and this was the overwhelming favourite amongst the graduate students, whether they originated in quarrels about who had been sleeping with whom twenty-five years earlier.

Problems of persuasion

Suppose we feel ready to offer policy advice either on some specific issue or more generally by drawing attention to some potentially significant area which is emerging. Our ideas may be excellent and our politics and morals beyond reproach, but we still somehow have to persuade those who have the power to make decisions that we think are right and that they should follow our advice.

Our job is the twofold one of trying to have good and justifiable ideas and then persuading other people of their truth and relevance. As academics we are committed to the notion of rational argument. However, in the late twentieth century we have become world-weary and most of us doubt that simply 'Speaking Truth to Power' is sufficient. We have to get other people to act on ideas when we think we have something worth acting on. It is fairly obvious that they will only do so if the ideas suggested fit into the general presuppositions held by the decisionmaker anyway. We live in a world in which both the powerful and the powerless think within pre-existing conceptual frameworks and which they will usually retain.

There are two broad influences on policymakers which are hard to remove where the policymaker is anyone who can make a decision about the future of the social system and not just members of a governmental system. These are the influence of selfishness and the influence of orthodoxy. I shall deal with them in turn.

Selfishness

It may seem dreadfully realist in its implications but few people want to be significantly damaged by any rearrangement of power or wealth. They do not want to lose power and perhaps even more they do not want to lose wealth. The resistance of the tobacco industry to cutting back on its products after the overwhelmingly clear evidence that they are extremely dangerous suggests that morality caves in before significant amounts of money. We have to realise that those who control vast resources whether through the state or through business activity are likely to want to continue to do so, while consumers, such as ourselves, still want to consume as much and normally more than they do at the moment. The Game of Power in this broad sense consists of distributing the benefits of power now, while retaining power for the future.

This is clear in the case of the environment. Despite the occasional voices to the contrary, it is beyond reasonable doubt that there is a serious environmental problem. While there are genuine uncertainties and people can rationally disagree about timing and the precise consequences, if only because of the extreme complexity of

the environmental system, global warming, amongst other things, is with us. It will get worse over the decades and is very hard to reverse except over very long periods of several lifetimes. We know of several things we could do to alleviate this and we could significantly check the adverse developments. There have been some moves but it seems widely agreed by everyone who does not profit by the *status quo* that these are grossly inadequate. The reason is, quite simply, that a lot of rich and powerful people would be inconvenienced and made less wealthy.

How, then, do we 'speak truth to power'? Pointing out that there is a problem is not sufficient. One possibility is that one can try to persuade the powerful that they should adopt a longer time perspective while remembering that the powerful here are not just governments. Another alternative is to provide profitable alternatives to the current activity. In the case of the arms trade, instead of providing export guarantees for the sale of armaments to unsavoury regimes, we could provide subsidies for shifting into some different forms of production. Both involve intervention in the market mechanism though this would interfere with this government's simpleminded devotion to market mechanisms which is shared by so many others. However, the British arms industry is not a totally natural market growth but one which has been tended lovingly by a succession of governments who have strong objections to subsidies in most other contexts. The problem, however, is not easy. It is not just speaking truth to power but of persuading power to act against its self interest which is perhaps usually done by arranging countervailing power. All this is hampered by the prevailing view that governments, who might very well present countervailing power to business or could at least arrange for it to come about, are becoming less able to do things. Governments fall supine before the powers of globalization and international markets and pretend there is not much they can do. Perhaps the truth governments need to heed is that they need not be so powerless after all.

Orthodoxy

By and large, people are trapped within the orthodoxy of the time and it is very hard to get away from it. Thus, today it is hard to recommend any economic policy which does not make appropriate obeisance to market forces. We can talk 'truth to power' till the cows come home but it will make little difference if the truth told is uncongenial. Hans Morgenthau spoke truth as he saw it in a moderately congenial form in the earlier days of the Cold War. However, once he spoke uncomfortable truths the powers that be did not want to listen. I once heard him talk briefly about his campaigning in opposition to the Vietnam War. He ended by saying 'I might as well have collected butterflies'.¹¹ I thought then and still think today, that that was one of the saddest things I have heard said in a professional context.

Though it happened long ago, it is instructive to look at the role of Keynes in what is now almost universally regarded as the fiasco of the return to the Gold Standard at the pre-war parity in 1925. Keynes must surely be the ideal for the

¹¹ International School on Disarmament and Research in Conflicts (ISADARCO) Conference in Urbino, Italy, 1974.

academic policy adviser. He had a brilliant mind and made great theoretical and practical advances; he was an effective and often terrifying controversialist. He was someone who had spent time as an influential official in the Treasury. From Eton and King's, he had a pedigree which would count today and counted much more earlier in the century. It was a pedigree that made him central in the cultural, intellectual and academic establishment. He argued before the event that a return to the Gold Standard would be foolish. A few days before it happened he was present at a small dinner party given by Winston Churchill who was the Chancellor of the Exchequer, which was convened to argue the issue out.¹² How much closer can one get to power? Churchill himself was somewhat sympathetic but orthodoxy was not to be denied. Keynes responded characteristically by writing a pamphlet, *The Economic Consequences of Mr Churchill*¹³ in much the same way as he had responded to his disappointments over the economic aspects of the treaty of Versailles. He commented later, in 1931, that his writings were 'the croakings of a Cassandra who could never influence the course of events in time'.¹⁴ For all his social and intellectual centrality, Keynes free-thinking and heterodoxy reduced his direct policy influence in the interwar period.

For all his moaning, Keynes had a lot of influence at various points in his life. It peaked in the period of great flux as the Second World War drew to a close and when the postwar monetary arrangements were formulated. At that period, orthodoxy and heterodoxy were intertwined and it was hard to tell one from the other. Keynes' ideas were of even greater importance in defining the future orthodoxy in the quarter of a century after the Second World War. He defined the range of thought which was proper for an insider to think and pushed the formerly orthodox views out into the heterodox cold, from which, regrettably, they made such a successful comeback. Keynes can be a disappointment for our profession for some of his failures (though we should not exaggerate these). However he is an encouragement for those of us who do not see ourselves whispering into the ears of cabinet ministers. We may sigh sadly at our lack of influence today but if we are setting the terms of debate for, say 2025, we may in fact be more influential than we think.

As I said above, we like to think our trade is in rational argument, showing that A follows from B and thereafter we must conclude C all on the basis of some facts which have some acknowledged degree of ambiguity. However, people are often not persuaded by argument alone. Non-rational factors are equally important, particularly so over issues of violence. Sometimes we make a direct play to the emotions, but very often we play to the psychological susceptibilities of the relevant people. To imply that a course of action is 'weak' and another 'strong' is a good way of pushing people into the strong camp. The British economy has suffered sadly on occasion because of the false associations of a 'strong pound'. Many of the difficulties of thinking about nuclear weapons are because opposition has been seen as wimpish. Orthodoxy, blended with a need to feel tough, imposed a policy whose rational basis was in my view doubtful and which reached its peak in France with De Gaulle. I do not wish to dwell on this non-rational factor in persuasion. However, the very fact

¹² Robert Skidelsky, *John Maynard Keynes: Hopes Betrayed 1883–1920, volume 1* (London: Macmillan, 1983). Roy Jenkins, *The Chancellors* (London: Macmillan, 1998).

¹³ J. M. Keynes, *The Economic Consequences of Mr Churchill* (London: the Hogarth Press, 1925).

¹⁴ Robert Skidelsky, *John Maynard Keynes: the Economist as Saviour 1921–1937, volume 2* (London: Macmillan, 1992), p. 14.

that policy presentations are full of pretty pictures and bright colours far beyond what are necessary for the clear appreciation of the data suggests that, at best, the rational content of the persuasive act is only partial.

What does the social world need to look like for policy to be possible?

Some necessary conditions

The tacit assumption so far is that policy is possible. That is, that decisions can be made which actually do make a difference to how the world behaves. Few of us, even those far on the structuralist wing, are totally determinist so, to some degree, we believe that policy can make a difference. The question is, 'when can actions by a given actor make a predictable difference and under what circumstances?' Three things about this question are immediately obvious. First, the answer is not self-evident: secondly, some aspects of the question involve problems of high theory, epistemology and ontology: thirdly, it is crucially important as far as policy is concerned. However, those seeking to answer the question are unlikely to be speaking truth to power—or for that matter shouting or whispering it, at least while concerned with the question. Many of the arguments might well seem tedious to those who are not involved in them. Their implications, however, once worked out, would be of central importance.

Unfortunately I cannot provide an answer to the question posed in my main heading. I have only a sketchy idea what an answer would look like. However, I will suggest three central issues which need to be considered. First, policy requires some degree of prediction; secondly, as an extension of this, we need to know when the social world is vulnerable to some change in input, such as the decision of an actor, and when it is so stable that nothing much can alter it; finally, there has to be some degree of agreement as to how we see the social world.

Prediction, quite properly, is a topic of great embarrassment to social scientists as we are so bad at it and not just in International Relations. By prediction, I mean not only the extrapolation of statistical trends and things of that sort which are often unsuccessful, but also confident assertions that balances of power bring peace and so on. That is, any assertion that one state of affairs leads to another and will do so in the future qualifies as a prediction. Predictions that a certain trend will be increased or damped by some form of policy is something in which we are particularly interested. Many of the predictions on the environment are of this form. Unfortunately, however bad we are at prediction, we have to do it if we are to pretend to be carrying out a rational policy. To choose one course of action rather than another involves having 'expectations' about the consequences of the action as opposed to the consequences of the alternatives. If Britain sells arms to another state, the government should have some expectation about the effect this will have on the likelihood of war in the region and the effects on desirable things such as the well-being of the inhabitants. If we have no such rationally-based expectations then we are simply being irresponsible, as I suspect we often are.

Knowing when we can intervene effectively in social systems, and when not, is also a form of prediction. Social systems are at times very stable so that policy can

only be a matter of tinkering at the fringes. At other times, there are opportunities for major changes. Unfortunately we are not always sure which is which. Indeed, just as with the form of prediction above, we are often very bad at it. There are times when we can reasonably assume there is a lot of decision latitude such as after major wars. Likewise we would suppose that the political and social systems of the USSR and the Eastern bloc were malleable after the collapse of the communist systems, even though we failed to predict the collapse of the Soviet Union. The criticism of our profession and related professions is not that we did not predict the fall of the USSR but that it was not even on the agenda. Further, this seems the norm. The Falklands war came by surprise to most people, as did the invasion of Kuwait, as did the Iranian Revolution and practically any other event where there was a major ruction in the political system. The break-up of the former Yugoslavia is a counter instance. The idea that there would be problems at some stage after the death of Tito had been on the agenda for a long time. That the detailed events were not predicted is not something about which we need feel too guilty.

It is crucial for us to know when social systems are stable and resistant to change as opposed to when they are unstable and amenable to change. Further we need to know the direction in which any acts of policy will direct that change. Our lack of skill at this does not alter the fact that it is central if we are serious about doing anything. Our current state of ignorance leads to one of two conclusions. Either we cannot find it out, in which case International Relations is at best of modest policy use. Alternatively, it is a suitable topic for research. Doubtless the research can be pursued in many different ways and, given our abysmal ignorance, we need a diversity of research programmes where one, at least, would involve the analytical discussions of systems involving a certain amount of mathematics. I know this is not a popular thing to say. Many in the United Kingdom are sympathetic to the English School with the apparent belief of its adherents that mathematics is not a suitable occupation for a gentleman. (In those days before feminism, the idea of a lady mathematician would have been far beyond their conceptual domain). However unpalatable this view may be to some, I find it congenial. I believe such systemic analysis may in the long run yield big rewards. One thing seems certain: that these areas must be tackled, but they cannot be dealt with at a commonsense level alone.

Notice that I am not asserting that prediction is possible, though in fact I think some limited forms are. I am merely saying that, if it is not, then a rational basis for policy or policy advice is missing and we should stop pretending. Then, perhaps, our best advice to a policymaker is to buy a nice set of dice and rely on them. Perhaps BISA could set up a special subcommittee to advise on the technical and aesthetic aspects of the dice. Having helped choose the dice, they can then disband the profession.

The last of my points is that this analysis requires some limited objectivity in one's observations of the social world. I suppose this is the most contentious of the claims of the putative policy analyst. However, without something which we can call at least 'intersubjective agreement' (a term I picked up in the positivist Cambridge of the nineteen-fifties) I do not see that we can make meaningful social choices at all. We could rather selfishly pursue the interests of our group and therefore not care how other people perceived our actions or whether they liked them or not. However, we are not going to get far if we do not have some idea of what other people want and will do. It does not matter whether we call what they want their perceptual

framework, their world view, their utility function or whatever. From a purely pragmatic and practical point of view, some insight into other people's views of the world is necessary. Consequently most theories of decision, including the rational choice theories, have within them some notion of people having insights into other people. They are none of them, therefore, truly behavioural, which is why I call myself an empiricist rather than a positivist.

Further if we have moral views based on the principle that the welfare of people other than ourselves matter, we must also have some insight into what people want. I shall not argue the point beyond saying that there are surely some basic human experiences which are universal. Thus, we can assume that practically everyone, irrespective of their culture, does not want to starve, to be bombed, to die in childbirth or for their infant children to die. Policies which are likely to reduce such things will be generally welcomed. An unwillingness to empathize with other people on the grounds that today's middle-class Westerners live a privileged life in which these things rarely happen, quickly becomes farcical and at times irresponsible.

Positivism

Some of these arguments must seem dangerously positivistic in the loose sense in which International Relations scholars use the term 'positivism'. My approach, though not in a narrow sense positivistic, is vulnerable to some of the same objections as more dogmatic positivist approaches. Therefore, I want to look briefly at some misunderstandings about positivism. Some interpretations of what I would prefer to call the social scientific approach are often very misleading and relate to the logical positivism of the Vienna Circle. Not long ago I read a thesis in which Morris Schlick, who died in 1936, was cited as an exemplar of the positivist tradition. This seemed to be going rather far back in time, though it occurred to me that Schlick might have been chosen as a gentle reminder that he had met his early death at the hands of a doctoral candidate whose thesis he had failed. Nevertheless, lots of things have happened in this positivist tradition since then. In general, people writing in a social scientific mode are well aware of them. I do not mean that there do not exist today scholars whose philosophy of science could have come straight from 1936. However, it seems foolish to characterize an intellectual position by reference to its least sophisticated proponents. If you want to know more about my approaches to positivism, empiricism and the rest you could to your profit read, and to my profit buy, my book *Causes and Consequences in International Relations*.¹⁵

One of the most puzzling features of many accounts of the more recent history of the discipline of international relations is the frequent reference to the 'positivist hegemony'. When, oh when did this take place? Why did no-one tell me at the time? When I hear this I feel a bit like the Prime Minister of Iceland being chided for imperialism by the British. Some sort of case can be made that what I, to the horror of many of my friends, would regard as serious social science was practised and respected in the United States in a way it never has been in the British International

¹⁵ Michael Nicholson, *Causes and Consequences in International Relations: A Conceptual Study* (London: Pinter/Cassell, 1996).

Relations community. However, as a hegemony of the positivists, this has been grossly exaggerated. Whatever the situation in the United States, I cannot for the life of me remember any period in Britain where positivism was more than dimly visible on the International Relations horizon. If there was a hegemony in the United Kingdom, surely it was that of the historians, as a jaundiced observer might still think. Of course, a handful of positivists (as broadly construed) exist here. They gather together over a drink from time to time to lament the sorry state of International Relations in Britain (they can all fit round a small table, probably in a pub—something far smaller than the ‘high table of an Oxbridge College’ which the English School enjoyed in its heyday).¹⁶ No doubt they are assumed to be plotting the overthrow of Western Civilization, or, if not that, at least the English School which many seem to think is much the same thing. As subverters of the intellectual order, they are unconvincing: as hegemon, they are non-existent. Only by defining positivism so broadly as to make it effectively a meaningless term which would include most of the membership of BISA, could we talk of a positivist hegemony in British International Relations. Ole Wæver attributes this to the humanities tradition from which British International Relations has been primarily drawn.¹⁷ This is very plausible, though it does not explain why the broader field of Political Science in Britain, which draws its members from a similar intellectual tradition, has been more forthcoming than International Relations as far as social scientific methodologies are concerned.

One can make a better case for a positivist hegemony in the United States, but even there it is exaggerated. Morgenthau’s was the most widely read text¹⁸ during this supposed dominance, and Morgenthau can be made a positivist only on grotesquely broad definitions. Nevertheless, I can well believe the allegations that job references have been written which were less than enthusiastic about people who did not follow the positivist line. Since University departments have existed, people have been treated unfairly from time to time. Even more have believed themselves to have been treated unfairly. However, I have no reason to suppose that positivists are any worse than anyone else as supporters of their own. I would argue it is not serious even if particular departments are dominated by one school of thought provided teaching, particularly at undergraduate level, is balanced. Indeed, certainly in the pre e-mail days, such dominances were almost necessary to establish various research programmes and traditions. I would only worry if all departments in the country were dominated by the same orthodoxy. One of the merits of the large university system is that there is room for different schools of thought in different places. Of course it is nice to have access to the wine cellars and kitchens of Oxford and Cambridge, but a host of empirical observations have confirmed that port after dinner is not a necessary condition for successful academic research.

¹⁶ Tim Dunne, *Inventing International Society: A History of the English School* (London: Macmillan, 1998).

¹⁷ Ole Wæver, ‘The Sociology of a Not So International Discipline: American and European Developments in International Relations’, *International Organization*, 25 (Autumn 1998), pp. 687–727).

¹⁸ Hans Morgenthau, *Politics Among Nations: The Struggle for Power and Peace* (New York: McGraw Hill, 6th edn. revised by Kenneth W. Thompson, 1985; first edn. 1948).

The cult of the tenured victim

The trouble is we all suffer from a certain degree of paranoia. The guest editors of the issue of the *International Studies Quarterly*, which was given over by the positivist hegemons to the postmodernists,¹⁹ have been teased and chided for talking of the 'voices from exile'. Exile is no doubt relative. While I can sympathize with those who are cast away amongst the sheep, cattle, wheat, mountains and other such things, it must be bearable on a full professor's salary. To many colleagues in other parts of the world today, and even more until recently, such a fate would seem delightful. As socialism fades, at least in certain quarters, the champagne socialist hands over to the champagne 'pomo' as the defender of the wretched of the earth.

Paranoia is not just a characteristic of postmodernists, left wingers or whatever. A distinguished American formal theorist in Political Science complained to me recently of the dire straits some formal theorists had been placed in due to the machinations of some fellow scholars who were more interested in collecting statistical data than in probing the subtle depths of theory. A less than statistically significant, and certainly not randomly selected collection of cases were cited of creative scholars who had been deprived of their just deserts by this political chicanery. Anecdotal if not statistically the evidence was convincing. Being of a theological disposition he phrased the problem in terms of ultimate truths. He argued that while it is clear that all mortals are sinful, there are none so sinful as those engaged in collecting statistical data on political events. The full professorship at a leading university that my friend and theological adviser occupies suggests that these machinations were not wholly successful, which leads one to think that the data gatherers were not merely sinful but, what is worse, incompetently sinful. But the sense of being a member of a beleaguered minority shone out and was all too familiar. Likewise, I once picked up a book in Blackwells called *Against the Current*²⁰ expecting an outburst from the periphery of academia. It turned out to be by Isaiah Berlin, amongst other things a Fellow of All Souls, sometime Warden of Wolfson College and then about to become the President of the British Academy. His editor claims he 'sails manfully against the current' which is rather like sailing manfully against a light tide in a battleship.

Perhaps we all like to see ourselves as romantic outsiders, cast out upon the hilltops, calling out neglected truths—though always taking care to be safely home for tea and tenure. The truth, I think, is best expressed in a distortion of the words of a former inhabitant of the Parisian Left Bank 'Hegemons are other people'.²¹

Misunderstandings about positivism

However, there is a view held by many in this social scientific tradition that analysis and prescription should be held apart. At the very least, Popper's 'demarcation

¹⁹ Richard K. Ashley and R. J. B. Walker 'Spreading the Language of Exile: Dissident Thought in International Studies', Special Issue of *International Studies Quarterly*, 34:3 (1991).

²⁰ Isaiah Berlin, *Against the Current: Essays in the History of Ideas* (London: Hogarth Press, 1979).

²¹ Jean-Paul Sartre, 'L'Enfer, c'est les Autres', *Huis Clos*.

principle'²² between scientific and other statements provides a useful discipline, though I would argue more for it than that. There is a conceptual distinction between an analysis of the world as it is, and prescriptions of the world as it ought to be. This was certainly the view of Lewis Fry Richardson, a towering but neglected intellect amongst modern British writers on International Relations, and characterizes his work.²³ If anyone sailed against the current, surely it was Richardson. But it was Richardson also who recognized that while knowledge is neutral its applications are not. When the Rand Corporation wanted to publish his work, he refused, as it implied a potentially warlike use for the work he had done as a Quaker scientist. He thought of suppressing his work. The work is in the finest tradition of value neutrality, and unlike many, I use the phrase 'fine tradition' as applied here without irony. However, he was fully aware of the value context in which such work is asserted. The whole empirical tradition in peace research would be a nonsense if this were not a widely held view. However, the temptation, which Richardson faced in thinking of suppressing his views, is one which makes all academics shudder with horror. We think that truth is an absolute which should always be asserted and tremble before the moral dilemmas of publishing such truths when they can be taken up and used for what we regard as evil purposes.

Another of the strange interpretations of the intellectual history of our discipline is that this supposed positivist hegemony meant that moral argument ceased and was devalued until recently. This is generalized further to include the assertion that normative theory has been revived in recent years after a blight of decades. It is widely accepted that Rawls made political philosophy respectable again²⁴—but this was in 1971 which is quite a long time ago. With the time-lag which it seems that we require in International Relations before catching up with the intellectual fashions which convulse the rest of the chattering classes, we finally caught up with the problems of political philosophy in our area too. It seems that we are all eager readers of the *New York Review of Books*, but only get our copies ten years late.

However, there was much moral argument going on and had been since the Second World War, as William Wallace recognises. A great deal of it involved arguments about the moral problems of nuclear deterrence. This was surely both understandable and proper. The at one time influential *Bulletin of the Atomic Scientists* was significant here both in analysis and moral debate. Anatol Rapoport provoked a lot of disapproval amongst his enemies and embarrassment amongst his friends for his refusal to work with supporters of nuclear deterrence, support which he held to put them beyond the moral pale. Michael Walzer's work on the morality of war²⁵ is famous, and published in 1977, over two decades ago. The whole Peace Research movement was passionately interested in the moral problems of war. Normative theory might have been concerned, perhaps obsessively concerned, with issues of peace and war and particularly nuclear war but the reasons are not hard to

²² Karl Popper, *The Logic of Scientific Discovery*.

²³ Lewis Fry Richardson, *Collected Works, volume 2: Quantitative Psychology and Studies of Conflict* (Cambridge: Cambridge University Press, 1993); Oliver Ashford, *Prophet or Professor: The Life and Work of Lewis Fry Richardson* (London: Hilger, 1985); Michael Nicholson, 'Lewis Fry Richardson and the Analysis of the Causes of War', *British Journal of Political Science*, 25:3 (July 1999), pp. 541–63.

²⁴ John Rawls, *The Theory of Justice* (Cambridge, MA.: Belknap Press, 1971).

²⁵ Michael Walzer, *Just and Unjust Wars: A Moral Argument with Historical Illustrations* (New York: Basic Books, 1992, 2nd edn., first published 1977).

find nor is their justification. Human life might have ended. The beetles are allegedly the creatures most adept at biological survival and we might have surrendered the earth to them. At least, then, we would have had an uncontested hegemon. I am not denying that there has been a revival of interest in other forms of normative theory but to pretend that there was no normative theory or that it was cast out on the periphery is simply false. To pretend further that this was driven to the margins by the positivists, sour, dour people who were only interested in technicalities is even more farcical. The empirical wing of the Peace Research movement, Deutsch, Rapoport, Singer, Boulding, Russett and the rest were and are profoundly moved by moral concerns, but believed that value-free science could provide this. Debates took place which are eerily reminiscent of current debates. Herman Schmid, referred to earlier, produced the counter claim which was so avidly debated. In his own summary of his paper²⁶ he wrote 'The value problems of a value-oriented discipline are discussed, with the conclusion that peace research in the final analysis is based on a negative concept of peace in line with the institutional needs of the power-holders of the international system' (p. 231). What more could one of today's critical theorists want? People talked about such things in 1968, saying, of course, much the same things as they say today.

More significant in some ways was Peter Winch who first published *The Idea of a Social Science*²⁷ in 1958, a book which was quickly very influential as it still is. It disputed the whole basis of social science as it was practised in the positivist/empiricist mode and was a central part of the debates on the philosophy of the social sciences.²⁸

Finally, many people today, who should know better, are happily ready to assert that positivism, as I shall still politely call it, is an inherently conservative doctrine in that it deals with what is the case rather than what can be the case. At best, 'problem solving changes' are the best it can cope with (though I do not think there is anything particularly wrong in solving problems). This seems to me rather like saying that to research the aetiology of AIDS is to imply approval of AIDS. The whole aim of research for change is to find out what is changeable about any system, whether human or otherwise, and what are the constant factors. Critical theorists, just as much as anyone else, need to know how and when interventions can be made into any social system such that they will be effective. Further they need to know the consequences of the actions. The bigger the changes, and presumably critical theorists want big changes, the greater is the required degree of knowledge about social systems. We can only know what is and what is not possible by looking at what is the case and seeing how it can be re-arranged. If 'freedom is the recognition of constraints', we must explore these constraints, find their limits and stretch these limits in our search for better worlds. This can be done only by an investigation of the world as it is to see where it can be changed and where not. Hopeful worlds, where hope is not based on hard analysis of what can be done, must be looked at with scepticism.

²⁶ Schmid, 'Politics and Peace Research'.

²⁷ Peter Winch, *The Idea of a Social Science* (London: Routledge, 2nd edn. 1990, 1st edn. 1958).

²⁸ Winch's views are discussed sympathetically in Hollis and Smith, *Understanding and Explanation in International Relations*. I discuss them more critically in Michael Nicholson, *The Scientific Analysis of Social Behaviour: A Defence of Empiricism in International Relations* (London: Pinter, 1983) and *Causes and Consequences in International Relations: A Conceptual Study* (London: Pinter/Cassell, 1996).

Conclusion

What do I conclude from this? That William Wallace can still talk truth to power? That Steve Smith can continue to reflect on problems of epistemology and ontology? That even I can carry on working on rational choice, at least if I am discrete about it? Obviously yes. Somewhere we have to think we are talking about the real world and that that real world can be changed in some beneficial directions. Not to believe this would be intolerable. However, we should not all be doing the same things, not because we are indecisive liberals unable to make up our own minds but because a profession must spread over a broad range if it is to be effective and no single person can occupy more than a few points in such a range. We must be allowed our private moments and private quarrels, providing at some stage they come out into the open, expressed in reasonably comprehensible language.

For some reason, there has been a theological overtone to the whole of this debate which I admit I have continued. It seems appropriate, therefore, to remind you of St Augustine's prayer 'Lord, make me chaste—but not yet'. Likewise, at least if I were given to prayer, I would ask 'Lord, make me a defunct economist—but not yet'.